from its surface, but then so strongly that under no circumstances whatever can any body come into actual contact with it.

This appears to be the only constitution we can imagine for a rigid-elastic body. And now that we have got it, the best thing we can do is to get rid of the rigid nucleus altogether, and substitute for it an atom of Boscovich—a mathematical point endowed with mass and with powers of acting at a distance on other atoms.

But Boltzmann's molecules are not absolutely rigid. He admits that they vibrate after collisions, and that their vibrations are of several different types, as the spectroscope tells us. But still he tries to make us believe that these vibrations are of small importance as regards the principal part of the motion of the molecules. He compares them to billiard balls, which, when they strike each other, vibrate for a short time, but soon give up the energy of their vibration to the air, which carries far and wide the sound of the click of the balls.

In like manner, the light emitted by the molecules shows that their internal vibrations after each collision are quickly given up to the luminiferous ether.

If we were to suppose that at ordinary temperatures the collisions are not severe enough to produce any internal vibrations, and that these occur only at temperatures like that of the electric spark, at which we cannot make measurements of specific heat, we might, perhaps, reconcile the spectroscopic results with what we know about specific heat.

But the fixed position of the bright lines of a gas shows that the vibrations are isochronous, and therefore that the forces which they call into play vary directly as the relative displacements, and if this be the character of the forces, all impacts, however slight, will produce vibrations.

Besides this, even at ordinary temperatures, in certain gases, such as iodine gas and nitrous acid, absorption bands exist, which indicate that the molecules are set into internal vibration by the incident light.

The molecules, therefore, are capable, as Boltzmann points out, of exchanging energy with the ether.

But we cannot force the ether into the service of our theory so as to take from the molecules their energy of internal vibration and give it back to them as energy of translation. It cannot in any way interfere with the ratio between these two kinds of energy which Boltzmann himself has established. All it can do is to take up its own due proportion of energy according to the number of its degrees of freedom.

We leave it to the authors of the "Unseen Universe" to follow out the consequences of this statement.

J. CLERK MAXWELL

OUR BOOK SHELF

Report on the Progress and Condition of the Royal Gardens at Kew during the Year 1876. (Clowes and Sons.)

SIR JOSEPH HOOKER'S annual report on the Royal Gardens, Kew, for 1876, has just been issued. It is a pamphlet of some thirty-three pages, and is a considerable improvement on the reports of former years. It deals most fully with new plants of economic interest, whether such have been actually received or sent from the Royal Gardens, or have formed the subject of correspondence with foreign or colonial governments. It is eminently satisfactory to know that such useful plants as the Para rubber (*Hevea brasiliensis*), the ipecacuanha (*Cephælis ipecacuanha*), the Liberian coffee (*Coffea liberica*), and others, have been most successfully introduced into India and other countries, through the instrumentality of Kew. Of the 70,000 seeds of the *Hevea* received at Kew about the middle of June last year, all of which we are told were at once sown, and though closely packed together, covered a space of over 300 square feet so soon as August 12th following, upwards of 1,900 living plants, raised from these seeds, were transmitted to Ceylon in thirty-eight Wardian cases, 90 per cent. of the whole consignment reaching Dr. Thwaites in excellent condition. So rapid was the germination of these seeds at Kew that some had actually started into growth on the fourth day after sowing, and many in a few days reached a height of eighteen inches. It has been arranged that these young plants shall "be nursed and established in Ceylon for subsequent transmission through the Indian Gardens to Assam, Burma, and other hot damp provinces of India proper." Besides those sent to India, smaller quantities of plants have also been despatched to the west coast of Africa, Burma, Dominica, Jamaica, Java, Queensland, Singapore, and Trinidad. With regard to ipecacuanha, though Dr. King reports that he fears it cannot be grown so far north in India as Bengal, it is nevertheless in some situations capable of rapid and extensive cultivation, and the roots grown in India have been proved to be quite as efficacious in a medicinal point of view as those from the best districts of South America. In the matter of Liberian coffee, the wide and general extension of this new kind in coffee-growing countries bids fair, in many parts, to entirely supersede the old and better known *Coffea arabica*. Sir Joseph Hooker reports the receipt of numerous favourable notices of the plant, and quotes "two from opposite sides of the world," namely Ceylon and Dominica. With reference to diseases affecting coffee plants—which it is hoped the more sturdy habit of the Liberian kind will help it in some measure to resist—a very exhaustive notice is furnished, which is not only of much interest in a scientific point of view, but cannot fail to be valuable to coffee-planters themselves. It will, moreover, no doubt be the means of causing more careful observations to be made by residents on coffee estates or in coffee-growing countries into the nature and habits of diseases which are still more or less obscure.

Considerable additions are reported to the Museums and Herbarium, the new building for the accommodation of the latter collection being now in a very advanced state. The new Laboratory, which has been erected at the expense of T. J. Phillips Joddrell, Esq., is reported as having been completed during the year, and though not fully provided with the necessary equipment at the time the report was written, has been already, as our readers are aware, used by Dr. Tyndall in several of his recent experiments and researches.

Two new features of the report which we have not already mentioned are—first, the introduction of plates, one being a figure of the new Liberian coffee plant, and the other a view and ground-plan of the Laboratory; and second, the publication of the report, at a charge of sixpence, by Messrs. Clowes and Sons.

Natural History Transactions of Northumberland and Durham, vol. v., part 3. (Williams and Norgate, 1877.)

THIS part is by no means the least valuable of these transactions; on the contrary, it will rank high, owing to the contributions of Dr. Embleton and Mr. Atthey on the structure of the Labyrinthodonts, and the eight excellent plates by which their papers are illustrated. The illustrations of Loxomma and Anthracosaunis are as complete and instructive as any that have yet been pub-

lished of British Labyrinthodonts. The authors, however, do not recognise the articular surfaces on the exoccipitals of Loxomma as the two condyles; and they speak of a concave articular surface as taking the place of a condyle or condyles on the basioccipital bone. The condyles in all Amphibia are produced by the exoccipital bones, and such a character is not a special evidence of the affinity of Loxomma with fishes. The number also contains several interesting papers on local natural history and antiquities, and the address of the president, the Rev. G. Rowe Hall.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The "Inflexible"

MY attention has been called to an article on the *Inflexible*, in NATURE (vol. xvi., p. 201), and I shall be much obliged by your inserting a few remarks, which I shall make as short as possible. On the general subject of the article I do not propose, nor would it be proper for me, to say a word. I am concerned only with the concluding remarks of the writer on a letter of mine to the *Times*. Nothing has appeared to me more astonishing than the use, or rather abuse, which is occasionally made of the report of Lord Dufferin's committee on ships' designs. If their authority can be claimed for any statement, I see on all sides a readiness to claim it. Should anything they have said militate against a favourite view, their authority is depreciated, and a comparison is sometimes invidiously drawn between the supposed opinion of the majority and that of an intelligent minority. Now if I be included with the unintelligent majority, I am quite content to find myself in such good company; but if, on the contrary, I am included in the minority, I utterly and absolutely refuse a compliment at the expense of my distinguished colleagues, with whom I shall always esteem it no small honour to have served. In fact I am not aware of any scientific point on which that committee was not unanimous. The writer of the article in question in common with many others, seems to have entirely mistaken the position of that committee. He seems to think their duty was to make their report a treatise on Naval Architecture. The absurdity of such a notion is apparent on the face of it. In fact they were required to give an opinion on certain designs of ships submitted to them as to their being in accordance with the latest developments of the theory of naval architecture. It was no part of their duty to descant on the principles which were successfully applied in such designs; but undoubtedly if they observed that in any direction caution was necessary, they were bound to remark it. In none of the designs was there any indication of a tendency to curtail initial and maximum stability of their due proportions; had there been they would certainly not have failed to call attention to it. But while they found the design of the *Devastation* in all respects sound, they yet thought it desirable that the range of stability should in future designs be somewhat enlarged. In recommending such enlargement they by no means committed themselves to any such absurd *dictum* as the writer imagines—that range of stability is all that is requisite for the safety of a ship. But as I have already said, to have laid down all the other requisites of a good ship would have been to write a treatise.

Again, whatever credit according to some, or discredit according to others, is due to the design of a ship like the *Inflexible* with an armour-plated central citadel with unarmoured bow and stern, that credit or discredit cannot be justly imputed to the committee. Mr. Reed, in his evidence, had brought a design with some of the leading features of such a ship before them, and it occupied a considerable share of their attention. Now what do they say on this subject?—"It is not by any means certain that some method may not be devised of securing the requisite reserve buoyancy by other means than armour plating." And after giving a sketch of what such a ship would be, they conclude thus:—"In the absence of any practical experience of the effect of large shells or of torpedoes upon such a structure as we have in view, it is impossible to say with confidence that the object aimed at would be thus attained, but, *if it were*, consequences of so much importance and value would follow that we think it right to *indicate this line of inquiry as worthy of experimental investigation*."

How far such a bare suggestion of experimental inquiry is from the recommendation of such a structure for adoption must be evident to your readers without further comment.

United University Club, Pall Mall, July 20 JOSEPH WOOLLEY

[The above letter from Dr. Woolley is what might have been expected from a man of his eminence in the science of naval architecture, writing under the restraint of his nomination by the Government to a membership of the Committee which is to report upon the stability of the *Inflexible*. It is no doubt to the concluding words of our first article on this subject (NATURE, vol. xvi. p. 203) that Dr. Woolley's letter refers, and we at once admit that there is very great force in the argument which he now employs. The particular point in question is a very simple one. In his letter published in the *Times* of July 19, Mr. Barnaby wrote:—"According to our estimate the ship, when fully armed, stored for fighting, and manned, will have, independently of the unarmoured ends—*i.e.*, supposing them not to exist—a range of stability of 48 deg. The Committee on Designs considered that 40 deg. was sufficient range for a sea-going unmasted ship." On the following day a letter appeared from Mr. Reed commenting on the impropriety of assuming the *non-existence* of the ends, pointing out that it was 50 deg. and not 40 deg. that the Committee spoke of as the *minimum* angle of vanishing stability, and adding that when the Committee put forward "*range* of stability" as "the one measure of safety" to be considered, "they stated the most dangerous doctrine which probably has ever been propounded in connection with the science of naval architecture." Now, on reconsidering the whole question, we are inclined to think that these words were not, in point of fact, quite fair to the Committee, because there was probably no member of the Committee who would have asserted or admitted that "range" was the one and only measure of safety to be considered. Dr. Woolley, Mr. Froude, Sir W. Thomson, and probably some other members of the Committee, doubtless knew perfectly well that the length of G Z from point to point was not only as important as "range," but far more important in all cases of limited range; and it is now obvious, with the present letter of Dr. Woolley before us, that the absence of any reference to the fact is attributable to the limited extent of the Committee's inquiry. There is great force in the remark that it was no part of the duty of the Committee to compose a treatise on naval architecture. On the other hand we are bound to deny that our remarks were penned under a contrary impression. Our view is that the use to which Mr. Barnaby has put the Report of the Committee proves that the scientific men who composed it would have done well to have employed more guarded language, and to have recognised in some manner the insufficiency of range only as a measure of safety. When they are found speaking of a certain angle of vanishing stability as being "sufficient to ensure the safety" of ships, it must be admitted, even by Dr. Woolley and his colleagues, that some risk of misconstruction was incurred. That misconstruction, or perhaps we ought in this case to say misuse—or even "abuse," as Dr. Woolley expresses it—has occurred in the present case is manifest, because Mr. Barnaby seized hold of the Committee's dictum as to range, and ignored altogether the very serious question of the amount of the stability. What makes the matter more important than usual in the present case is that the curve of stability due to the citadel of the *Inflexible* only is, no doubt, a low and flat curve, G Z being everywhere so small that in order to bring the stability up to a safe amount its range